whereas the stimulus of a particular use or injury may not be received. If, however, the latter be received, the acquirement arises just as inevitably as the innate character. Thus if the child receives a like injury, it reproduces the scar on its parent's nose as certainly as the nose itself. If the nose is inborn and inherited, then the scar is inborn and inherited in the same sense.

Had the true nature of the distinction between innate and acquired characters been realised, had it been realised that the difference is one of stimuli, not of innateness or inheritability, and that acquirements are just as much products of evolution as innate characters, it is impossible that the controversy as to the alleged "transmission" of the former could have endured so long as it did. In effect, it was maintained by Lamarckians that a character (*e.g.* a scar) which the parent was able to acquire in a certain way (as a reaction to injury) because a long course of evolution had rendered such acquisition possible to the members of his race is reproduced by the child in a different category of characters, and in a way (as a reaction to nutriment) that no member of his race had ever acquired it before, and with which, therefore, evolution had nothing to do. An actual miracle was supposed to happen, the miraculous nature of which was concealed under a misuse of terms.

At the present day the majority of biologists are apt to regard "acquirements" as mere accidents, as things inferior to and less worthy consideration than "inborn" traits. Very little study has been given to the evolution of the power of making acquirements, especially use-acquirements, and hardly any attempt has been made to ascertain in what proportions the "normal" individual of any species is compounded respectively of innate and acquired traits. Lloyd Morgan, Baldwin, and Osborn have certainly dealt with this power under the name of "plasticity." But plasticity is not the same as growth, as development, and the fact that they have regarded acquirements as useful to the species mainly as affording time and opportunity for the evolution of corresponding inborn traits indicates an adherence, even if only a modified adherence, to the prevailing biological view. The evidence seems clear that animals low in the scale of life have little or no power of making use-acquirements, but that this power increases as species are more highly placed until in man the main difference between the infant and the adult is due to the use-acquirements made by the latter during development. The power of making use-acquirements is present only in structures where it is useful, and only to an extent that is useful. Great adaptability is thus conferred on the individual, for he develops only those traits which are useful to him in his particular environment, and is burdened with no others. We have a special name, memory, for the power of making mental use-acquirements. Memory is nothing other than the power or faculty of storing mental experiences, and so adding to the mental growth. It is strictly analogous to the faculty of storing physical experiences, and so adding to the physical growth. Without memory there could be feeling and (instinctive) emotion, but no thought, for the materials of thought would be lacking. Animals low in the scale of life appear to have little or no memory; they are guided more or less entirely by instinct. Man is intelligent and adaptable because he has a memory. He is the most intelligent of animals because he has the largest faculty for storing experiences. Memory, the *power* of learning, develops under the stimulus of nutriment, but intelligence and reason develop under the stimulus of use. They are amongst the *contents* of memory. We learn to think and reason just as surely as we learn the facts about which we think and reason. Reason, therefore, is an "acquirement."

Probably no problem in biology is of greater theoretical interest than that of the evolution of the power of making use-acquirements. Certainly no problem is of nearly such practical importance as that of determining the extent to which the individual develops, on the one hand, under the stimulus of nutrition, and, on the other, under the stimulus of use. From the times of Lamarck, Spencer, and Romanes, biologists have very generally assumed that use tends to cause development in all the structures of all animals, but that the amount of this modification is trivial. As a fact, use causes development in only some structures in some animals, and the major part of the development of the human being is due to it. If, for example, biologists had ascertained and were agreed as to the amount of this development, we should know to what extent races and generations of men differ "innately" and to what extent by acquirement, and therefore what effect could be produced by this or that system of mental training. Educationists could then apply this knowledge to the training of the young. At present the basis of bed-rock fact is lacking, and biology is shorn of much of the practical importance which is its right.

I venture to write this letter in the hope of directing attention to one, at least, of the great problems of biology which are neglected under present fashions. Experiment itself, for example, loses much of its value unless the worker has clear and comprehensive notions concerning the subject with which he deals.

G. ARCHDALL REID.

The Melanic Variety of the "Peppered Moth."

MR. SPICER asks (January 16, p. 247), among other questions, "how does the 'peppered moth' contrive to appear in the black country hatched with sooty wings that harmonise with the now smoke-stained bark whereon he must rest?" His point, I conceive, is that the melanic variety is due in some unexplained way to the inheritance of acquired characters.

If Mr. Spicer found that an actor whom he had seen perform the part of Hamlet on Tuesday was cast for Macbeth on the Wednesday, he would not necessarily, I suppose, conclude that the actor had added the part of Macbeth to his repertoire during the intervening time. Now there is more than a possibility that the black coloration of the variety Doubledayaria may in like manner be a repertoire pattern of the "peppered moth" evolved in the remote ages of the history of the species. The dark form is not necessarily atavistic in the general acceptation of the term, as it may only have been developed by some stocks of the species in a more or less restricted portion of its range, the stocks in question having reverted when the factor that put a premium on blackness gave place to the original conditions of their habitat. The facts of mimicry prove that the germ plasm of the Lepidoptera can carry more than one distinctive pattern, and the temperature experiments of Standfuss and Merrifield suggest that such latency may extend over long periods of the insect's history.

Mr. Spicer has tacitly assumed that the variety is confined to the black country, but this is by no means the case. The dark form is, I believe, taken in the Black Forest in Germany; certainly it occurs in Denmark, and records from our own southern counties are not wanting. It is by no means uncommon in and round London, and has been taken as far out as Brentwood and Bexley, both of which are outside the smoke limit as regards soot-stained bark. In the last-named district, my friend Mr. Newman has taken melanic forms of several other Geometriid moths in addition to var. Doubledayaria.

There seems to be no doubt as to the increase of melanism among the tree-resting species of Lepidoptera in certain districts of England during the past fifty years, but this increase is apparent outside the actual smoke-stained area, though not perhaps beyond the range of darker bark owing to the destruction of the lichens—a cause that may have operated locally on more than one occasion during the life-history of the species quite irrespective of a sooty civilisation.

Apart from lichens, even a change in the species of trees composing a forest might have a marked effect on the cryptic coloration of the bark-resting species of moths in the locality. Birch would favour a pale coloration; oak, cherry, and especially the thorns, a darker one; beech, with its dense shade and wide range of bark coloration, a darker or lighter pattern, according to the dampness of the situation and whether the particular species emerged before or after its full leafage was attained.

The time during which the dark form could have been evolved from the normal coloration of the species by the action, direct or otherwise, of smoke is less than a

century, say from fifty to seventy-five generations—presumably a quite inadequate period for the evolution *and fixing* of the form by the selection of small chance variations. Certainly, if the analogy of language in the human race is permissible, the number of generations is far short of what would be required to impress any character on the heredity of a species *by the inheritance of acquired characters*, even if we could find any reasonable connection between soot-stained bark and darkened wings for the purposes of the theory.

But gradual adaptation during the present epoch does not fit the facts for another reason. The darkening, if gradual, would have been noticed by entomologists, as is the case with *Aplecta nebulosa* in Delamere Forest and *Hybernia leucophaearia* in Epping Forest. The species would be a beautiful example of a mutation if it were not for the fact that intermediates, though rare, have a puzzling habit of turning up; and, what is more serious, a careful examination of the melanic forms reveals the fact that on the upper margin of the hind wings, where they are covered by the fore wings when the moth assumes its normal resting position, there is an area of the original pale coloration. As in the reverse case of the exposed tip of the underside of the fore wings of many butterflies being coloured quite differently from the rest of the wing area, in order that it may match the cryptic pattern on the underside of the hind wings, the retention of the pale area in var. Doubledayaria can only be accounted for by the supposition that the variability is the work of natural selection.

If the above reasoning be correct, the black variety must either be regarded as the recurrence of a pattern slowly evolved in some previous epoch, or we must consider it as an example of the working of Weismann's germinal selection. The needs of cryptic adjustment to environment having put a premium upon darker, but not necessarily black forms, the determinants of the darkened characters tend by the operation of selection within the germ to increase progressively to a point where they are cut off by the operation of natural selection upon the individual. As a consequence, a few rare examples will always be thrown having such a progressive character in excess, and should any rare and sudden chance such as is afforded to melanism by our smoky civilisation occur, an enormous premium is placed upon the survival of their offspring.

A. BACOT.

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Inductance in Parallel Wires.

A PROBLEM of some considerable importance to the practical engineer or physicist is that of calculating the effective self-induction of a circuit consisting of two parallel wires, the one being the return of the other. When the wires are not very close together, and their current is either steady or only very slowly alternating, satisfactory results are known to be given by the formula

$$\frac{L}{l}=2\log\frac{c^2}{ab}+\tfrac{1}{2}(\mu_1+\mu_2),$$

where L is the self-induction of a length l, c the distance between the wires, which have radii a, b, and μ_1, μ_2 the permeabilities of their materials. But if the current oscillates rapidly, this formula fails to give even approximately correct results. Now in many practical problems, such, for example, as the measurement of small inductances not greater than 1000 microhenries, it is necessary to employ long leads to keep them at some considerable distance from bridge and other circuits. A knowledge of the self-induction of such leads is very desirable. Some results which I have recently obtained are capable of finding this quantity in most useful cases, and it may prove of use to give a short statement of them, pending more detailed publication.

The self-induction has a simple expression only if the two wires be equal in radius. In this case it takes the form

$$\frac{L}{l}=4\log\frac{c}{a}+\frac{4\mu}{x}\cdot\frac{\operatorname{ber}x\,\operatorname{ber}'x-\operatorname{bei}x\,\operatorname{bei}'x}{(\operatorname{ber}'x)^2+(\operatorname{bei}'x)^2},$$

where ber x, bei x are the functions introduced by Lord Kelvin, and subsequently tabulated (*vide* Presidential Address to the Institution of Electrical Engineers, 1889).

If $\frac{n}{2\pi}$ be the frequency of alternation per second, σ the specific resistance of a wire, μ its permeability, then

$$x=2a\sqrt{\frac{\pi\mu n}{\sigma}}.$$

This formula, passing naturally into the former when the frequency is small, becomes less accurate as c decreases and as the frequency or radius of a wire increases. So far as the first cause is concerned, it is subject to an error of not more than 1 per cent. when $c=10a$, and 4 per cent. if $c=5a$. If $c=3a$, which is the limiting closeness for most practical purposes, the error is about 10 per cent., which is not usually too great. The other causes of error may be considered together. The per cent. error they produce is of order $100.\frac{n^2a^2}{V^2}$, where $V=3\cdot10^{11}$. Practically, a is never more than about 2 millimetres, and thus, with a frequency of a hundred million per second, the error is not more than one-tenth per cent. The range of application of the formula is therefore extremely wide. A formula equally accurate may be given when the wires are unequal, but it is somewhat cumbrous.

J. W. NICHOLSON.

Trinity College, Cambridge, January 21.

Stock Frost or Ground Ice.

DURING the recent frosty weather the subject of what is locally called "stock frost" has been much to the front in this neighbourhood. This phenomenon is known to the scientific world, I believe, as "ground ice," and the circumstances in which it appears and disappears present to the ordinary observer a very great many puzzling features.

I should be exceedingly glad if some of your readers would kindly give me, through the columns of NATURE, their opinion on several points which puzzle and interest me and others in connection with "stock" or ground ice.

(1) I wish to know, first of all, what are the *essential* conditions for the formation of ground ice on the bed of a river?

(2) Is it essential, or does it favour the formation of "ground" ice, that there should be no surface ice? We notice that when a very cold and very strong north-east wind is blowing, violently agitating the surface water, there is no surface ice, but a formation of ground ice at the bottom of the river.

(3) What are the circumstances to which is due the presence of ice-cold water at the bottom of a river, cold enough to be precipitated into ice?

This ice-cold water cannot reach the bottom of the river by gravitation, because its density is inferior to that of water at a higher level. To what, then, is due this cold temperature on the river bed?

(4) Can the bulk of water in the river bed be a conductor of cold from the surface to the bottom of the river in any other way than that of the mechanical action of running water? I assume that when ground ice appears in a river the whole of the water above it is of an ice-cold temperature, but it has not formed into ice because of the lack of the ice-precipitating conditions which exist on the bed of the river.

(5) Do the conditions necessary for the formation of ground ice operate more favourably in ice-cold still water or in that which is agitated, say, by passing through a mill? My own observation is that ground ice appears nearer to a mill on its upper side than on its lower side, and I want to know the reason of this.

There is quite a long list of questions which might be asked in connection with the formation of ground ice, but I fear that I have already trespassed too much upon your space.

JOHN J. HAMPSON.

Costessey Vicarage, near Norwich, January 20.